THE JULY EFFECT: LABOR TURNOVER AND PRODUCTIVITY IN TEACHING HOSPITALS

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ABSTRACT

The impact of labor turnover on productivity has received a great deal of attention in the literature on organizations. We consider this issue by examining the annual July turnover of residents in teaching hospitals in the United States. Our empirical setting is particularly well-suited for this analysis due to the exogenous nature of the turnover and the readily available data on both resource utilization and product quality. Using patient-level data from roughly 750 U.S. hospitals per year over the period from 1993 to 1997, we find that the annual resident turnover each July results in declines in hospital productivity that last for most of the last half of the calendar year. Relative to non-teaching hospitals, we identify significant increases in both the average length of patient stay (i.e., greater resource utilization) and patient mortality rates (i.e., lower product quality) for those facilities that rely on residents for the provision of medical services. We also find that the effect with respect to mortality is not monotonic in a hospital's reliance on residents for the provision of care. The most intensive teaching hospitals manage to avoid significant effects on patient mortality, suggesting some returns to scale in managing labor turnover.

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I. INTRODUCTION

The managers of most firms must eventually deal with turnover in their respective labor forces. As employees retire and others are brought in to replace them, managers must determine how to transfer knowledge from one generation of workers to the next as seamlessly as possible. Ensuring that such transfers occur smoothly becomes critical to maintaining productivity.

Turnover at a given firm typically corresponds to one of three general patterns. First, a firm may face a continuous stream of turnover in which employees leave and are replaced by new workers at various points throughout the year. There is never any one particular time during the year, however, when managers at these firms are required to retrain a large portion of their workforce at once. Second, a firm may bring on new employees at discrete points in the year. For example, law and consulting firms tend to start most of their new employees in late summer or early fall. These new employees must all be trained and melded into the work force at one time. While employees begin working at discrete points, departures occur in a roughly continuous manner throughout the year. Finally, a firm may have new employees start and seasoned employees leave at discrete points during the year. Given the number of individuals transitioning either into or out of employment at a specific point in time, this pattern may raise concerns about adverse effects on productivity.

In this paper, we focus on the third class of employee transitions described above at one type of firm—teaching hospitals. Teaching hospitals represent a particularly dramatic example of discrete turnover, as the inflow of new employees and the outflow of experienced employees occurs at the same time every year. Specifically, teaching hospitals rely on medical residents to provide a significant amount of patient care. These residencies typically last from three-to-five years depending on a physician's area of specialization. At the beginning of every July, the most senior residents move on to permanent medical positions or fellowships at other hospitals, and recent medical school graduates arrive as first-year residents, also known as interns. This turnover leads to a significant lack of continuity and a discrete reduction in the average experience of the labor force at teaching hospitals every summer. In addition, this changeover may disrupt established teams of doctors and other caregivers within hospitals. Either of these effects—a decline in the experience of the average doctor on the medical staff and the lack of familiarity within teams—may have potentially troubling consequences for the productivity of teaching hospitals.

This "July effect" (also referred to as the "July phenomenon") is often mentioned in the lore of medical professionals. Many physicians have, perhaps jokingly, counseled patients not to get sick in July. As of yet in the medical literature, however, any identified July effect has been limited to declines in hospital efficiency (i.e. higher costs or lengths of hospital stay) without any significant impact on clinical outcomes, such as mortality.

We examine the impact of the July turnover on hospital productivity using all patient admissions from a large, multi-state sample of American hospitals over a five-year period. Previous studies have utilized much smaller sets of data or different empirical methodologies, and, as we discuss below, issues concerning study design may explain the differences between our results and those from prior analyses. Using our national sample, we find significant negative effects of the residency turnover not only on hospital efficiency (as measured by average length of stay (LOS)), but also on quality (as measured by risk-adjusted mortality rates). Over some range, these effects appear to be increasing in the degree to which a hospital relies on residents (as measured by the number of residents per hospital bed). Nevertheless, there is evidence of eventual returns to scale in avoiding the negative effect of this turnover, at least with respect to mortality rates. Specifically, those hospitals with the highest levels of residents per bed (i.e., high teaching intensity) appear to be less affected—in terms of mortality—by the July effect than facilities with low-to-medium teaching intensities.

The remainder of this paper is organized in five sections. Section II provides background on residency programs and a discussion of previous research. Section III describes the data, Section IV outlines our empirical methodology, and Section V presents and discusses our results. Section VI concludes.

II. BACKGROUND AND PREVIOUS RESEARCH

A. Background on Residency Programs

It is widely agreed that teaching hospitals have two primary objectives—the provision of high quality medical care and the training of new doctors. These related but distinct objectives overlap within medical residency programs. Medical school graduates in the United States apply for residencies at any of the roughly 800 teaching hospitals in the country. Depending on a physician's specialty, residencies typically last for three-to-five years, during which time residents represent an important piece of a hospital's system for delivering care.

Most areas within a teaching hospital have a staff of medical professionals that include attending physicians, residents, and medical students. Much of the care for patients is provided by a resident, who is supervised by the chief (i.e., most senior) resident in that field and an attending physician. On a typical day, each medical resident at a busy teaching hospital will be responsible for admitting, treating, and/or discharging roughly five patients.

Residency programs in the United States are structured like schools. Each class of residents enters together at the beginning of the academic year, and the senior members of the program all graduate together. For residency programs, the year begins and ends on July 1st. The annual transition, however, does not occur all on one day. Typically, hospitals will complete the entire transition over a two-to-three week period, lasting from the middle of June through the first week of July.

One might imagine that hospitals would transition the new interns into their positions slowly. Anecdotal evidence, however, suggests that each cohort of residents typically moves up one level and covers the entire set of responsibilities of the group it is replacing. As a result, on day one new interns may have the same responsibilities that the now-second-year residents had at the end of June (i.e., after they had a full year of experience).

The resulting potential turmoil in teaching hospitals as each new cohort of doctors becomes comfortable with new roles and responsibilities has led to the contention amongst many in the medical profession about the undesirability of ending up in a teaching hospital in July. Claridge *et al.* [2001] make the following observation about the residency changeover:

> During this time of year, there is clearly a feeling of apprehension among providers of health care, as well as among many patients. Within our particular institution there are attendings who specifically take their vacation in July because of the difficulty in working during this transition.

Gawande [2002] echoes these concerns:

> In medicine we have long faced a conflict between the imperative to give patients the best possible care and the need to provide novices with experience. Residencies attempt to mitigate potential harm through supervision and graduated responsibility…But there is still no getting around those first few unsteady times a young physician tries to put in a central line, remove a breast cancer, or sew together two segments of colon. No matter how many protections we put in place, on average these cases go less well with the novice than with someone experienced.

These anecdotal observations suggest the need for systematic analysis of the implications of this annual turnover for medical productivity.

B. Previous Research on the July Effect

Most of the medical literature on staffing and performance in teaching hospitals deals with issues concerning limitations on resident work hours[1] or differences in outcomes on weekends and weekdays[2]—two periods when the average level of on-duty-physician experience is expected to differ substantially. There exists a limited set of previous studies in the medical literature dealing with the July effect. Some studies find a link between the July turnover and hospital inefficiencies. Rich *et al*. [1993] examine several teaching hospitals in the Minneapolis area and find that doctors spend less money on diagnostic tests and pharmaceuticals as their experience increases throughout the year. These results only applied, however, for medical patients. The authors do not find such a pattern for surgery patients. Rich *et al*. [1993] use a difference-in-differences approach,

[1] Examples include Gaba and Howard [2002], Laine et al. [1993], Leach [2000], Steinbrook [2002], Thorpe [1990], and Weinstein [2002].

[2] Examples are Bell and Redelmeier [2001], Dobkin [2002], and Hendry [1981].

much as we do in this paper, to control for seasonal patterns. They utilize patient outcomes in non-teaching hospitals as a baseline from which to estimate the impact of the July turnover for teaching hospitals. They are able to identify some efficiency changes but are unable to find any evidence of mortality differences. Compared to our study, Rich *et al*. [1993] rely on data from a small number of hospitals. Perhaps with data from more facilities, the authors of that study may have identified an effect on outcomes.

While they do not directly test for the presence of a July effect, Griffith, Wilson *et al*. [1997] examine patterns in test ordering among physicians in the neonatal intensive care unit at a single hospital. They find that first-year interns are more likely to incur higher charges than their more experienced colleagues. In addition to its small sample size, this study is limited by the fact that it does not consider the effects of experience on medical outcomes.

A third study claims to reject the existence of a July effect on any dimension for the trauma unit at one particular hospital [Claridge *et al*., 2001]. This paper compares patient outcomes in April and May with those in July and August and does not identify any significant differences between the two periods. Given their study design, however, Claridge *et al*. [2001] are unable to control for seasonal variations in patient outcomes that could affect outcomes at all hospitals regardless of teaching status. For example, as we will illustrate later, patients admitted to hospitals in the winter have higher mortality rates than those admitted in the summer. Without some baseline to adjust for exogenous changes in patient outcomes, a comparison of outcomes for one hospital at two times of the year may be significantly confounded by these other, missing effects.

C. Previous Research on Worker Experience and Productivity

Several prior studies have examined the relationship between worker experience (either general or firm-specific) and productivity. Not surprisingly, many have suggested that worker productivity improves with experience [Levhari and Sheshinski, 1973; Maranto and Rodgers, 1984, Hellerstein and Neumark, 1995[3]]. Other studies, however, find that the positive relationship between experience and earnings is not well explained by increases in productivity [Medoff and Abraham, 1981; Dunson, 1985; Medoff and Abraham, 1985]. Still others suggest that labor turnover does not appear to have a negative effect on firm productivity in many settings where work is relatively standardized [Argote and Epple, 1990] or those where experience depreciates more rapidly than the rate of turnover [Argote, Beckman, and Epple, 1990]. Finally, some studies highlight the potentially beneficial impact of turnover on factors, such as technological transfer, that may improve productivity [Dalton and Todor, 1979].

A confounding factor in many analyses is the fact that various measures of experience are often endogenous. For example, more productive workers are likely to remain with a given company longer than less productive ones, creating potential bias due to unobservables. Brown and Medoff [1978] suggest that some portion of the positive relationship between unionization and productivity that they identify may be attributable to the greater stability of unionized workforces.[4] To the extent that

[3] Using data on Israeli manufacturing firms, Hellerstein and Neumark [1995] find that experience is positively related to both earnings and productivity. Nevertheless, they note that their estimates are sufficiently imprecise so as to preclude them from rejecting models in which wages rise faster or more slowly than productivity.

[4] While Clark [1980] finds a positive relationship between unionization and productivity, he suggests that additional evidence is required to establish the degree to which this relationship is explained by lower turnover. Freeman and Medoff [1984] provide a summary of the factors—including, but not limited to, lower turnover—that may explain this relationship.

unionization provides a relatively exogenous source of labor stability, one approach for identifying the impact of turnover on productivity would be comparisons of otherwise similar union and non-union firms. Our study takes a different approach to identifying this relationship. We focus on an empirical setting characterized by a regular pattern of exogenous turnover. Specifically, the annual turnover of residents occurs regardless of the underlying productivity of the individual physicians and hospitals involved.

III. DATA

The primary source of data for this analysis is the Healthcare Cost and Utilization Project (HCUP) National Inpatient Sample (NIS) for each year from 1993 (Release 2) to 1997 (Release 6).[5] NIS contains discharge-level data for all inpatient cases at a sample of roughly 20% of the community hospitals[6] in the United States. Depending on the year, NIS includes information for hospitals from between 17 and 22 states.

For each patient, NIS provides information on patient age and gender, expected primary payer (i.e., Medicare, Medicaid, private including HMO, self pay, no charge, and other), length of stay (LOS), total charges, and in-hospital mortality. In addition, NIS includes detailed data on a patient's principal and secondary diagnoses, principal and secondary procedures, and diagnosis-related group (DRG).

[5] The NIS database is administered by the Agency for Healthcare Research and Quality (AHRQ), previously known as the Agency for Health Care Policy and Research (AHCPR).

[6] The NIS definition of "community hospital" is the same as that used by the American Hospital Association (AHA): "…'all nonfederal, short-term, general, and other specialty hospitals, excluding hospital units of institutions.' Included among community hospitals are specialty hospitals such as obstertrics-gynecology, ear-nose-throat, short-term rehabilitation, orthopedic, and pediatric. Excluded are long-term hospitals, psychiatric hospitals, and alcoholism/chemical dependency treatment facilities [Healthcare Cost and Utilization Project, 1999]."

We link the NIS data with information from the AHA Annual Survey of Hospitals, which includes data on the operating and financial characteristics for more than 6,000 hospitals each year. In addition to several other items, the AHA database provides information on the number of hospital beds and full-time residents and interns at each facility in a given year. Using this information, we are able to construct our measure of teaching intensity—full-time residents and interns per hospital bed.

Due to fact that not every hospital in the United States reports its data to the AHA in a given year, our final sample of facilities is limited to those that appear in both the NIS and AHA databases. Table I presents the number of hospitals that appear in our sample and in the NIS by year. For each year and state, the table provides the number of hospitals appearing in the NIS and in our matched NIS-AHA sample. Most of the discrepancies between the matched sample and the NIS are due to the fact that certain states opted not to provide identifying information for specific facilities. As such, the AHA identifier required to link the AHA and NIS for those hospitals is not available. For example, South Carolina's decision not to provide identifying information for its hospitals decreased the number of facilities in the matched sample by between 34 and 52 per year. In other rare cases, a hospital may appear in the NIS but not the matched sample because that facility did not appear in the AHA data for a given year.

IV. EMPIRICAL METHODOLOGY

A. Hospital Categories

The source of identification in our empirical analysis is the varying degree to which certain types of hospitals rely on residents. Initially, we divide hospitals into three categories—non-teaching hospitals, minor teaching hospitals, and major teaching hospitals. Non-teaching hospitals are those that are not listed as teaching hospitals in the NIS. These facilities have few, if any, residents. As such, we would not expect them to be affected by the July changeover. Those hospitals that are listed as teaching hospitals in the NIS data are subdivided into two categories. Minor teaching hospitals are those teaching hospitals that have resident intensities (i.e., full-time residents per inpatient hospital bed) that are less than 0.25, while major teaching hospitals are those facilities with teaching intensities equal to or greater than 0.25. This threshold for resident intensity is used by the Medicare Payment Advisory Commission (MedPAC) to distinguish minor and major teaching facilities [Medicare Payment Advisory Commission, 2002].

The inclusion of non-teaching hospitals in this analysis allows us to control for seasonal changes in outcomes that occur for all types of hospitals. Figure I illustrates that, in the aggregate, both LOS and mortality, vary quite substantially throughout the calendar year. Productivity appears to decline in the winter months, as evidenced by increases in both LOS and mortality. In the analysis that follows, we will present data and figures that "de-season" the LOS and mortality patterns for teaching hospitals using the seasonal pattern of the non-teaching hospitals.

Table II presents descriptive statistics for each of the three hospital categories as well as for the entire sample. The first row illustrates the differences in average teaching intensity across the three groups. This average measure increases from 0.01 for non-teaching facilities to 0.10 and 0.50 for minor and major teaching hospitals, respectively. In terms of both measures of facility size—hospital beds and admissions per year—the hospitals get progressively larger as the level of teaching intensity increases. Teaching intensity is also correlated with the demographics of a hospital's patient base. In particular, non-teaching hospitals attract older patients than either type of teaching hospital. The average age for patients at non-teaching facilities is 48.0 versus 44.7 and 39.5 for minor and major teaching hospitals, respectively. Similarly, the percentage of admissions accounted for by Medicare falls from 38% for non-teaching hospitals to 24% for major teaching hospitals.

In addition to having younger patients, the major teaching hospitals in our sample also have a higher percentage of Medicaid patients than the other groups. Moving from non-teaching to minor teaching to major teaching, this percentage increases from 16% to 18% to 27%. This relationship is consistent with the fact that many teaching hospitals are located in densely populated cities.

The bottom portion of Table II presents information on the mortality rate and average length of stay (LOS) for each type of hospital. The values are not adjusted for differences in the severity of the case mix at each type of facility. While we perform more sophisticated risk-adjustment in our later analysis, here we simply present each rate for the entire population, as well as separately for patients younger than age 65 and those 65 and older. Average LOS increases with teaching intensity both for the entire population and each of the age groups. This trend is consistent with the claim that major teaching hospitals tend to attract the most complex cases among the three groups. Overall mortality, however, is highest for the non-teaching facilities—2.7% versus 2.4% for the minor and major teaching group. At first glance, this finding seems puzzling given the fact that the LOS data suggests that major teaching hospitals were attracting the most severe cases. Analysis of mortality by age category, however, reveals that, within each group, the mortality rate does increase with teach intensity. These latter results suggest that the higher overall mortality rate for non-teaching hospitals may be due to the high average age of their patients.

B. Basic Specification

Our multivariate analysis relies on a difference-in-differences framework that follows the relative changes in LOS and mortality for the three groups of hospitals over the course of the year. The basic specification takes the following form:

$$(1) \quad \begin{aligned} Y_{h,m,t} = {} & a_h + d_t + \mu_m + ß_1 \cdot MIN\_TCH_{h,m,t} + ß_2 \cdot MAJ\_TCH_{h,m,t} + \\ & \sum_{m=1}^{6} ß_{3m} \cdot (\mu_m \times MIN\_TCH_{h,m,t}) + \sum_{m=1}^{6} ß_{4m} \cdot (\mu_m \times MAJ\_TCH_{h,m,t}) + e_{h,m,t} \end{aligned}$$

where Y represents the dependent variable of interest (i.e., risk-adjusted mortality or risk-adjusted average LOS).

The first two terms on the right-hand side of (1) are vectors of fixed effects for hospital and year, respectively. The third term, m_m, represents a vector of fixed effects for six multi-month periods during the year—January through March, April through May, June, July through August, September through October, and November through December. Given that the residency changeover begins in late June for many hospitals, we isolate that month and then compare the change in the dependent variable from April-

May to July-August for teaching hospitals to the similar change for non-teaching hospitals to measure the impact of the July turnover.[7]

MIN_TCH and *MAJ_TCH* are indicators for minor and major teaching hospitals, respectively. The next two terms on the right-hand side of (1) are vectors of interactions between the teaching hospital categories and the month effects. The coefficients on the *MIN_TCH* (*MAJ_TCH*) interactions thus capture the extent to which any seasonal pattern that is found for minor (major) teaching hospitals differs from that for the non-teaching controls. Each of the observations in (1) is weighted by the total number of cases for the hospital-month pair to account for the fact that all of the dependent variables are averages. Finally, the standard errors are clustered by hospital to address potential lack of independence in the error term, $e_{h,m,t}$.

One limitation of (1) is that it restricts hospitals to three discrete categories in terms of their teaching intensities. This specification may not capture potential non-linearity in the relationship between teaching intensity and the dependent variables. We thus estimate a more flexible form of (1) that includes teaching intensity as a continuous variable. The form of this specification is:

$$(2)\quad \begin{aligned} Y_{h,m,t} = {} & a_h + d_t + \mu_m + ß_1 \cdot RES\_BED_{h,m,t} + ß_2 \cdot RES\_BED^2{}_{h,m,t} + \\ & \sum_{m=1}^{6} ß_{3m} \cdot (\mu_m \times RES\_BED_{h,m,t}) + \sum_{m=1}^{6} ß_{4m} \cdot (\mu_m \times RES\_BED^2{}_{h,m,t}) + e_{h,m,t} \end{aligned}$$

[7] Due to the fact that the residency changeover begins in 3rd and 4th weeks of June at several hospitals, mortality and LOS results for that month represent a mixture of outcomes from both before and after the transition. We thus use the comparison of July-August to April-May to measure the July effect. This difference captures the change in the dependent variables from the two complete months that precede the beginning of the changeover for *any* hospital to the two complete months that fall after its conclusion for *all* hospitals.

This model is identical to (1) with the exception that the teaching hospital categories are replaced with linear and quadratic values of teaching intensity (*RES_BED*). The interaction of this variable with the multi-month indicators captures the degree to which changes in teaching intensity impact the magnitude of the July effect.

C. Risk-Adjustment of Dependent Variables

As suggested by Table II, the average severity of patients likely differs across the three types of hospitals. To the extent that the differences in patient severity for major teaching, minor teaching, and non-teaching hospitals vary systematically over the course of the year, risk adjustment is required to ensure proper identification of any July effect. For example, to the degree that relatively healthy individuals in the population aged 65 and older move from cold climates in northeastern states—which tend to have a high concentration of teaching hospitals—to warmer southern and western states during the winter months, the mortality risk for the hospitalized population in the northeast will increase *ceteris paribus* during this period of the year.

The covariates in our risk-adjustment equation are patient age; age squared; gender; an indicator for Medicaid as the primary payment source; indicators for a patient's state of residence; interactions of the state indicators with both the linear and quadratic age terms; and the Charlson index—a measure of comorbidities that increase a patient's risk of mortality [Charlson *et al.*, 1987]. The Medicaid variable is included as a proxy for the patient's socioeconomic status.[8] The interactions of the state-of-residence

[8] With linear and quadratic terms for patient age included in the regression, we do not include a separate term for Medicare status. While it would be useful to include an indicator for HMO patients—who may be healthier, on average, than patients in other payer categories—the HCUP data does not distinguish HMO patients from those with other forms of private insurance (e.g., indemnity).

and age terms are included to control for the fact that the average severity of patients, conditional on age, may vary across geography.

Given that the in-hospital mortality variable is binary, we use logistic regression to obtain estimated probability of death for each patient discharge. For LOS, we use a simple linear regression to calculate predicted values. The risk-adjustment equations are run separately for each calendar year. The observed and expected values for mortality and LOS are then averaged by hospital and month. The risk-adjusted value of each dependent variable is calculated as the ratio of the observed-to-expected rate for a given hospital-year. For example, the risk-adjusted mortality rate ($RAMR_{h,m,t}$) is:

$$RAMR_{h,m,t} = \frac{OMR_{h,m,t}}{EMR_{h,m,t}} * \overline{OMR_t} \tag{2}$$

where $OMR_{h,m,t}$ and $EMR_{h,m,t}$ are the observed and expected mortality rates, respectively, for hospital h in month m of year t. OMR_t is the average mortality rate for the entire sample in year t and is used simply to normalize the value of $RAMR_{h,m,t}$.

V. RESULTS AND DISCUSSION

A. Results Using Discrete Categories of Teaching Intensity

Table III presents results from our estimation of (1), the basic regression using three discrete categories of teaching status**.** The coefficients in this table represent the change in the dependent variable for minor and major teaching hospitals relative to the change for non-teaching hospitals over the same period. As noted earlier, we use the period just prior to the resident turnover (April - May) as the baseline. A positive coefficient thus indicates that, on average, the hospital group in question experiences a larger increase in the outcome measure than does the non-teaching group over the same period of time. For example, the value of 0.044 for the September-October coefficient in Column 1 suggests that the *change* in LOS from April-May to September-October was 0.044 days greater for minor teaching than for non-teaching hospitals.

For our purposes, the coefficients of greatest interest are those in the period just following the resident turnover (i.e., July-August). In terms of LOS (Column 1), the July-August coefficient for minor teaching hospitals 0.022 but is not significantly different from zero at conventional levels. In the two subsequent periods (i.e., September-October and November-December), minor teaching hospitals exhibit a significant increase in average length of stay relative to the April-May baseline. This increase in LOS—0.044 days by September-October and 0.050 by November-December—is significant at the 5% level. We note that the average risk-adjusted LOS for minor teaching hospitals is 5.5 days. If we assume that LOS is proportional to hospital costs, these results suggest that costs increase by roughly 0.8% during the period from September through December.

Relative to smaller teaching hospitals, major teaching facilities show stronger evidence of a July effect with respect to LOS. Specifically, these facilities experience a positive and significant increase in LOS relative to non-teaching hospitals immediately following the July turnover, and the effect remains for approximately six to nine months. This increase appears to begin in June, as the estimated coefficient for that month (0.081) is significant at the 10% level.[9] The effect, however, appears to strengthen in terms of

[9] As noted earlier, June represents a mixture of days before and after the turnover at many hospitals. The coefficient on June may thus underestimate the immediate impact of the turnover.

both magnitude (0.129) and significance (1%) in the July-August period. The effect is roughly of the same magnitude for the remainder of the year and begins to dissipate in January-March, with the best performance of the major teaching hospitals being in the April-May period just prior to the annual turnover. The average risk-adjusted LOS for major teaching hospitals is 6.22 days, suggesting that the estimated increase in LOS of 0.129 days represents a 2.1% increase in hospital costs that remains for at least six months.

Concerning the pattern in LOS for the period from January through May, we note that LOS for major teaching hospitals in January-March—in addition to being significantly higher than that for April-May—is also significantly *smaller* than that for November-December. This decline in the coefficient from 0.142 to 0.077 to zero suggests evidence of learning over the course of the academic year at major teaching hospitals.

Column 2 presents the results for risk-adjusted mortality. For minor teaching hospitals, the mortality rate increases by of 0.051 percentage points (significant at the 10% level) in the July-August period. This change represents a 2.0% increase relative to the average risk-adjusted mortality rate of 2.61% for minor teaching hospitals in the sample. To put this change in perspective, it translates into an additional 0.7 deaths per month (over a two month period) given the average of 1,294 admissions per month at minor teaching hospitals.

As is the case with LOS, major teaching hospitals experience a larger and more significant increase in mortality immediately following the July turnover. The coefficient for June is 0.126 percentage points and is significant at the 1% level. In July-August, the effect is 0.185. For the September-October and November-December periods these increases are 0.147 and 0.175, respectively. All of the effects for the latter six months of the year are significant relative to the April-May base period, but are not statistically different from each other. The decline from November-December to January-March is significant at the 5% confidence level, indicating that the effect of the July turnover lasts for roughly six months before major teaching hospitals begin to return to their pre-transition level of productivity. Again, this result suggests the presence of learning at major teaching facilities over the course of the academic year. Despite this learning, the July turnover is associated with an additional 2.7 to 3.4 deaths per month for up to six months (based on an average of 1,830 admissions per month at major teaching facilities).

Based on the above analysis, it is not clear that all hospitals within each discrete category are affected equally. As a result, the findings thus far should not be interpreted as suggesting that the largest teaching hospitals are most susceptible to productivity declines surrounding the July turnover. In fact, additional results based on the continuous measure of teaching intensity imply that the effects identified above are not monotonic in residents per bed. In particular, we find that the most intensive of the major teaching hospitals appear to avoid a mortality increase over the summer. The effects on LOS remain for even the most intensive of the teaching hospitals. We discuss these results in more detail later in the paper.

B. Test for Patient Self-Selection

It is possible that some other phenomenon that fits the timing of our results could represent an alternative explanation for the productivity decline that we observe in major teaching hospitals. One potential hypothesis is that patients recognize July to be a time of turmoil for teaching hospitals and that those with choice (i.e., elective patients) decide to avoid those facilities at that time of the year. Of course, these elective patients are likely to be relatively healthier than those who lack choice regarding their admission to the hospital. This self-selection of the patient base could thus leave teaching hospitals with relatively sicker patient populations at precisely the time we estimate their outcomes to be declining. We would be mistaken, however, to attribute such a decline to a decrease in hospital performance.

We offer a test of this hypothesis in Column 3 of Table III. If patients are in fact self-selecting away from teaching hospitals in the summer, then teaching hospitals should experience a decline in their number of admissions relative to non-teaching hospitals during those months. We estimate a regression of the same form as the mortality and LOS regressions, but with the number of hospital admissions on the left-hand side. The results are not consistent with a self-selection story, as the coefficients are sometimes positive and sometimes negative. In particular, the coefficient in the July-August time period for major teaching hospitals—the period most critical for the analysis of the July effect—is positive. This effect is actually in the opposite direction of that which one would expect under the self-selection hypothesis.

C. High-Mortality Admissions

Given that low probability of in-hospital mortality across all diagnoses, we also estimate the regressions from the previous section using a sample of hospital admissions for which death is a more prevalent outcome. The admissions in our high-mortality subsample include all diagnosis-related groups (DRGs) for which the average in-hospital mortality rate nationwide was at least 5% for the period from 1993 to 1997.[10] These DRGs, listed in the Appendix, account for 18% of all hospital admissions in our sample and roughly 70% of the in-hospital deaths.

The results for the high-mortality subsample are presented in Table IV and are formatted identically to those in Table III. The overall pattern in the coefficients is qualitatively similar to that for the entire sample of admissions, with the exception that the effects for minor teaching hospitals are slightly more significant and better fit the timing of the resident turnover.

The results on LOS are in Column 1. For minor teaching hospitals, there is now an increase in average length of stay of 0.130 days in the July-August period and it is significant at the 5% level. We recall that, for the entire sample, this coefficient is positive, but not statistically significant. The effect for minor teaching hospitals remains through the end of the year but dissipates significantly by the November-December time period. By January-March, the effect is no longer significantly different from zero. The July effect of 0.13 additional days represents an increase of 1.4% relative to the baseline LOS of 9.32 days for high-mortality diagnoses at minor teaching hospitals.

For major teaching hospitals, LOS increases in the July-August time period by an average of 0.261 days. This effect, which is significant at the 1% level, remains through the January-March time period. While the September-October coefficient is not significantly higher than the July-August coefficient, it is greater than the November-

[10] Three DRGs with mortality rates in excess of 5% are excluded from our high-mortality sample. These are DRG 123 (circulatory disorders with acute myocardial infarction, patient expired), DRG 129 (cardiac arrest, unexplained), and DRG 457 (listed as no longer valid). DRG 123 was exclude because the mortality rate was, by definition, 100%. Similarly, DRGs 129 and 457 were eliminated due to their

December coefficient (at the 5% level). As is true throughout our estimations, the April-May time period is the best time of the year for major teaching hospitals relative to non-teaching hospitals. This pattern again suggests the presence of learning within major teaching hospitals with respect to the management of LOS.

With respect to mortality, minor teaching hospitals experience an increase in mortality in the July-August period of 0.27 percentage points (significant at the 5% level), and there is some indication that this effect may linger through the end of the year. The coefficient for the September-October months is positive, though not significant. In November-December, however, the effect is again positive and significantly different from the April-May baseline at the 10% level. The July-August increase of 0.27 percentage points represents approximately 0.6 additional deaths per month for a sample of patients that typically accounts for 22.4 deaths per month.

The high-mortality pattern for major teaching hospitals is similar to the pattern with all admissions. Major teaching hospitals experience an increase in mortality of 0.322 percentage points (significant at 10%) beginning in June. The effect increases to .485 percentage points (significant at 10%) in July-August and then lingers through the end of the year at roughly the same level. By the beginning of the year, major teaching hospitals return to their pre-transition level of productivity relative to non-teaching hospitals. These results suggest an additional 1.5 deaths per month for the average major teaching hospital (relative to the average of 33.0 deaths per month for this group of facilities). This effect lingers for approximately 6 months. We note that the estimated increase in mortality accounts for just under half of the estimated additional deaths from

extremely high mortality rates (79% and 57%, respectively). After these three DRGs, no other diagnosis had a mortality rate in excess of 33%.

the analysis on *all* diagnoses. The high-mortality admissions, however, account for roughly 70% of in-hospital deaths, suggesting that, proportionally, the resident turnover has a larger impact on the population of patients admitted with lower-mortality diagnoses.

We present results regarding the self-selection hypothesis in the third column. For minor teaching hospitals, we do find a pattern of hospital admissions consistent with patients selecting away from those hospitals during the summer. The magnitude of the effect is such that if all of the missing patients had entered the hospital and survived then the estimated increase in mortality would be significantly reduced. With the major teaching hospitals, however, we do not find a pattern consistent with self-selection. In fact, in the period of greatest interest (July-August), there is no estimated change in admissions relative to non-teaching hospitals.

D. Results Using A Continuous Measure of Teaching Intensity

The findings in the previous sections suggest that the negative impact of the July turnover on productivity is increasing in teaching intensity (i.e. major teaching hospitals are more affected than minor teaching hospitals). Intuitively, this makes sense, as teaching intensity captures the degree to which a hospital relies on residents and, therefore, should be correlated with the magnitude of the turmoil created by resident turnover. Nevertheless, a potential countervailing effect is that hospitals with larger teaching programs may have developed better infrastructures for managing this annual turnover.

To more directly test for this potential non-linearity in the relationship between teaching intensity and the magnitude of the July effect, we estimate (2), the model in which we replace our discrete hospital categories with a continuous measure of teaching intensity—residents per bed. We interact that measure with the multi-month periods employed in our previous analyses. To capture the potential returns to scale in teaching programs, we also include residents per bed squared and all of the corresponding interactions. As in the previous estimates, one would expect the linear-term interactions to be positive in the time periods in which the July effect occurs. To the extent that the most intensive teaching hospitals have mechanisms for managing the turnover, one would expect the squared-term interactions to be negative.

The results are presented in Table V. Columns 1 and 2 include estimates for the entire sample of admissions and Columns 3 and 4 provide the results for the high-mortality subsample. Our findings with respect to LOS are consistent with those in the previous analysis. LOS begins to increase in the July-August period, but there is no evidence that the effect on length of stay declines with higher teaching intensities. Specifically, the interactions with the squared terms are negative, but not significantly different from zero. In Figure II, we plot the estimated increase in July-August relative to April-May as a function of teaching intensity.

Our analysis of mortality, however, does provide evidence of returns to scale in terms of teaching hospital's ability to avoid major problems during the July turnover. We again find an increase in mortality during the second half of the year, but the interactions with the squared terms are now negative and statistically significant at the 1% level. Figure III illustrates that the estimated magnitude of the July effect with respect to mortality is increasing in teaching intensity until residents per bed reaches 0.50, a level equal to the mean teaching intensity for major teaching hospitals. Above 0.50, the estimated size of the July effect declines until it becomes insignificantly different from zero just below 0.80 residents per bed. Approximately 10% of the major teaching hospitals have intensities at levels that are equal to or greater than the 0.80 level at which the July effect becomes insignificant.

The results for high-mortality admissions are qualitatively similar, except that there is some evidence of economies of scale with respect to LOS as well as mortality. The July effect for LOS now begins to decline within the sample range, and becomes insignificantly different from zero at an intensity of roughly 0.90 residents/bed (Figure IV). The mortality estimates now peak slightly earlier—approximately 0.4 rather than 0.5 residents/bed—than in the full sample of patients, and the effect becomes insignificant just above 0.5 residents/bed (Figure V). Further, the mean value of the effect declines to zero at a slightly lower level of teaching intensity (0.8 versus 1.0 in the full sample).

VI. CONCLUSION

This study considers the impact of the July resident turnover on the productivity of teaching hospitals. We examine two different measures of hospital performance—mortality rates and LOS. We find that most teaching hospitals experience a significant increase in resource utilization—measured by average LOS—immediately following the July turnover, and that the effect appears to last for several months. We also find that teaching hospitals in the low-to-middle range of teaching intensity experience a significant increase in patient mortality over the same period. Those hospitals with the highest teaching intensities (i.e., the greatest reliance on residents for the provision of care), however, seem to avoid the disruption of the July effect with respect to changes in their average mortality rates. Nevertheless, these most intensive teaching facilities still exhibit a July effect in terms of increased resource utilization, as captured in their longer LOS beginning in July.

The magnitude of the estimated effects is substantial and appears to last for roughly six months. Using our discrete estimates, we find that average LOS—our proxy for resource utilization and cost—for the average, major teaching hospital increases by 2% following the July turnover. In addition, the average, major teaching hospital experiences an increase of 15-to-20 deaths per year attributable to the mortality increase following this changeover. Based on a total of roughly 200 major teaching hospitals in the United States, the July effect is thus associated with an additional 3,000 to 4,000 deaths per year. If these additional deaths were avoided, the annual average risk-adjusted mortality rate at major teaching hospitals would fall from 3.09% to between 3.00% and 3.02%. Given the magnitude of this effect, one might ask why it may go unnoticed by teaching hospitals. A possible answer to this question is that the July effect occurs at a time of the year when the overall trend in mortality is declining. As a result, an increase in mortality relative to non-teaching hospitals may not appear as an increase in absolute mortality.

Studies in the medical literature have established a decline in some hospital productivity measures over the summer months, but those effects have been limited to increases in hospital costs and LOS. To our knowledge, no previous study has identified a change in mortality rates as a result of the annual turnover due to the structure of residency programs.[11] We argue that our difference-in-differences approach with a large, national panel of hospitals over a multi-year period offers a unique opportunity to identify effects on mortality, which is a volatile measure that exhibits a cyclical pattern throughout the year for reasons beyond the structure of the residency programs. Additionally, many of the previous studies have focused on the most intensive of the major teaching hospitals and, as we have shown, those hospitals appear to be relatively unaffected with respect to mortality.

We are not arguing that an optimal residency system would result in no systematic change in productivity throughout the year. Presumably, no system can guarantee that residents will be as productive at the beginning of their first year as they will be at its end. Ultimately, the important question to answer is whether the decline in productivity for many teaching hospitals over the summer and fall is higher than necessary to train new physicians efficiently. To the extent that the July effect needs to be reduced, there are several adjustments to the process of resident turnover that might be

[11] Griffith, Rich *et al*. [1997] note the need for multi-site studies of the effect of house staff training on medical outcomes.

considered. For example, the results with respect to mortality for the low-to-medium intensity teaching hospitals suggest that those programs may be able to learn from their more intensive counterparts about how to manage residency transitions effectively. Ultimately, some programs may simply be the “wrong” size, in that they are big enough to make residents critical in the provision of care, but too small to justify the infrastructure necessary to manage the system properly.

Additionally, many residency programs currently subscribe to a regime where the new residents are depended upon to carry roughly a full patient load from their first day of work. It is conceivable that a system where the new residents begin their tenures in a more staggered manner over several months—or a system where the older generation of residents spends the first couple of months working more closely with the new residents prior to moving on to their new duties—might result in a smoother transition. We do not contend to know which of these systems is preferable or whether either would, in fact, be better or worse than the current system. Rather, we believe that our findings should encourage experts in this area to consider the available options.

APPENDIX: DRGs Included Among High-Mortality Admissions

DRG	Description	1993-1997 Cases	Deaths	Mortality Rate
Nervous System				
27	Traumatic Stupor & Coma, Coma >1 Hr	22,244	4,373	19.7%
2	Craniotomy For Trauma Age >17	17,474	2,875	16.5%
10	Nervous System Neoplasms W Cc	45,717	5,974	13.1%
14	Specific Cerebrovascular Disorders Except Tia	519,010	57,191	11.0%
1	Craniotomy Age >17 Except For Trauma	94,740	7,529	7.9%
34	Other Disorders Of Nervous System W Cc	37,803	2,803	7.4%
Eye				
64	Ear, Nose, Mouth & Throat Malignancy	8,481	1,293	15.3%
Respiratory System				
475	Respiratory System Diagnosis With Ventilator Support	164,790	54,066	32.8%
82	Respiratory Neoplasms	124,068	26,539	21.4%
87	Pulmonary Edema & Respiratory Failure	85,755	15,175	17.7%
79	Respiratory Infections & Inflammations Age >17 W Cc	284,013	37,592	13.2%
76	Other Resp System O.R. Procedures W Cc	71,136	5,798	8.2%
92	Interstitial Lung Disease W Cc	21,037	1,422	6.8%
89	Simple Pneumonia & Pleurisy Age >17 W Cc	602,392	35,158	5.8%
85	Pleural Effusion W Cc	30,161	1,747	5.8%
78	Pulmonary Embolism	55,281	2,836	5.1%
Circulatory System				
110	Major Cardiovascular Procedures W Cc	103,158	14,790	14.3%
103	Heart Transplant	1,744	160	9.2%
126	Acute & Subacute Endocarditis	12,866	1,081	8.4%
108	Other Cardiothoracic Procedures	25,373	2,052	8.1%
113	Amputation For Circ System Disorders Except Upper Limb & Toe	55,892	3,771	6.7%
137	Cardiac Congenital & Valvular Disorders Age 0-17	4,502	284	6.3%
104	Cardiac Valve & Oth Maj Cardiothoracic Proc W Card Cath	48,424	2,794	5.8%
105	Cardiac Valve & Oth Maj Cardiothoracic Proc W/O Card Cath	45,904	2,415	5.3%
115	Perm Pace Implnt W Ami,Hrt Fail Or Shock Or Aicd Lead Or Gen Proc	14,546	762	5.2%
127	Heart Failure & Shock	911,566	47,308	5.2%
144	Other Circulatory System Diagnoses W Cc	135,900	6,874	5.1%
Digestive System				
172	Digestive Malignancy W Cc	57,137	10,012	17.5%
170	Other Digestive System O.R. Procedures W Cc	25,556	3,134	12.3%
154	Stomach, Esophageal & Duodenal Procedures Age >17 W Cc	67,188	6,188	9.2%
173	Digestive Malignancy W/O Cc	5,176	423	8.2%
148	Major Small & Large Bowel Procedures W Cc	263,139	14,796	5.6%
Hepatobiliary System and Pancreas				
203	Malignancy Of Hepatobiliary System Or Pancreas	53,432	12,225	22.9%
201	Other Hepatobiliary Or Pancreas O.R. Procedures	3,529	515	14.6%
202	Cirrhosis & Alcoholic Hepatitis	75,769	8,807	11.6%
205	Disorders Of Liver Except Malig,Cirr,Alc Hepa W Cc	56,867	6,316	11.1%
200	Hepatobiliary Diagnostic Procedure For Non-Malignancy	4,529	435	9.6%
199	Hepatobiliary Diagnostic Procedure For Malignancy	4,743	431	9.1%
191	Pancreas, Liver & Shunt Procedures W Cc	29,229	2,368	8.1%
193	Biliary Tract Proc Except Only Cholecyst W Or W/O C.D.E. W Cc	15,089	834	5.5%
Musculoskeletal System				
239	Pathological Fractures & Musculoskeletal & Conn Tiss Malignancy	92,855	6,470	7.0%

APPENDIX: DRGs Included Among High-Mortality Admissions (Continued)

DRG	Description	1993-1997 Cases	Deaths	Mortality Rate
Skin, Subcutaneous Tissue, and Breast				
274	Malignant Breast Disorders W Cc	6,572	1,519	23.1%
275	Malignant Breast Disorders W/O Cc	1,001	74	7.4%
Endocrine, Nutritional, and Metabolic				
292	Other Endocrine, Nutrit & Metab O.R. Proc W Cc	7,963	430	5.4%
296	Nutritional & Misc Metabolic Disorders Age >17 W Cc	314,691	16,870	5.4%
Kidney and Urinary Tract				
318	Kidney & Urinary Tract Neoplasms W Cc	10,663	1,604	15.0%
316	Renal Failure	117,214	13,507	11.5%
319	Kidney & Urinary Tract Neoplasms W/O Cc	1,243	78	6.3%
Male Reproductive System				
346	Malignancy, Male Reproductive System, W Cc	7,988	1,410	17.6%
347	Malignancy, Male Reproductive System, W/O Cc	1,001	94	9.4%
Female Reproductive System				
366	Malignancy, Female Reproductive System W Cc	10,476	1,763	16.8%
367	Malignancy, Female Reproductive System W/O Cc	2,106	133	6.3%
Newborns and Neonates				
385	Neonates, Died Or Transferred To Another Acute Care Facility	75,699	15,890	21.0%
Myeloproliferative Diseases and Disorders/Neoplasms				
473	Acute Leukemia W/O Major O.R. Procedure Age >17	19,164	4,843	25.3%
413	Other Myeloprolif Dis Or Poorly Diff Neopl Diag W Cc	16,231	4,052	25.0%
414	Other Myeloprolif Dis Or Poorly Diff Neopl Diag W/O Cc	2,667	458	17.2%
403	Lymphoma & Non-Acute Leukemia W Cc	62,362	10,466	16.8%
401	Lymphoma & Non-Acute Leukemia W Other O.R. Proc W Cc	12,036	943	7.8%
406	Myeloprolif Disord Or Poorly Diff Neopl W Maj O.R.Proc W Cc	7,929	532	6.7%
404	Lymphoma & Non-Acute Leukemia W/O Cc	10,265	636	6.2%
400	Lymphoma & Leukemia W Major O.R. Procedure	19,206	996	5.2%
Infestious and Parasitic Diseases				
416	Septicemia Age >17	293,059	46,624	15.9%
415	O.R. Procedure For Infectious & Parasitic Diseases	81,345	6,756	8.3%
423	Other Infectious & Parasitic Diseases Diagnoses	26,508	1,401	5.3%
Injuries, Poisonings, and Toxic Effects of Drugs				
454	Other Injury, Poisoning & Toxic Effect Diag W Cc	15,701	943	6.0%
Factors Influencing Health Status				
465	Aftercare W History Of Malignancy As Secondary Diagnosis	2,924	242	8.3%
466	Aftercare W/O History Of Malignancy As Secondary Diagnosis	38,459	2,040	5.3%
Multiple Significant Trauma				
484	Craniotomy For Multiple Significant Trauma	3,015	837	27.8%
486	Other O.R. Procedures For Multiple Significant Trauma	28,521	3,439	12.1%
487	Other Multiple Significant Trauma	23,712	1,837	7.7%
HIV Infections				
488	Hiv W Extensive O.R. Procedure	10,619	1,294	12.2%
489	Hiv W Major Related Condition	117,677	14,018	11.9%
490	Hiv W Or W/O Other Related Condition	31,376	1,976	6.3%
Other				
483	Tracheostomy Except For Face,Mouth & Neck Diagnoses	77,233	22,798	29.5%
472	**Unspecified**	1,265	373	29.5%
480	Liver Transplant	3,386	362	10.7%
481	Bone Marrow Transplant	10,542	855	8.1%
468	Extensive O.R. Procedure Unrelated To Principal Diagnosis	133,142	10,233	7.7%
	SUBTOTAL	**5,903,176**	**609,945**	**10.3%**

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Table I: Number of Hospitals in the Sample and the NIS by State and Year, 1993-1997

	1993		***1994***		***1995***		***1996***		***1997***	
State	**Sample**	**Full NIS**	**Sample**	**Full NIS**	**Sample**	**Full NIS**	**Sample**	**Full NIS**	**Sample**	**Full NIS**
Arizona	13	13	12	12	15	15	15	15	14	14
California	95	96	101	102	104	105	102	103	106	107
Colorado	27	28	21	22	21	22	21	21	18	18
Connecticut	7	7	7	7	9	9	8	8	9	9
Florida	165	166	162	163	140	141	137	138		117
Georgia										115
Hawaii										3
Illinois	75	75	77	77	73	73	72	72	73	73
Iowa	70	70	64	64	54	54	53	53	52	52
Kansas		72		71		61		60		62
Maryland	40	40	42	42	39	39	39	39	35	35
Massachusetts	30	30	27	27	25	25	19	19	18	18
Missouri					48	49	46	47	44	44
New Jersey	20	20	19	19	18	18	17	17	19	19
New York	60	60	62	62	59	59	58	58	56	56
Oregon	19	19	19	19	17	17	17	17	16	16
Pennsylvania	57	57	53	53	51	51	50	50	52	52
South Carolina		52		51		46		41		34
Tennessee						52		50		64
Utah									13	13
Washington	23	23	21	21	22	22	22	22	20	20
Wisconsin	85	85	92	92	80	80	76	76	71	71
Total Hospitals	786	913	779	904	775	938	752	906	616	1012
Total States	15	17	15	17	16	19	16	19	16	22

Table II: Descriptive Statistics by Hospital Type, 1993-1997

	Non-Teaching		**Minor Teaching**		**Major Teaching**		**Full Sample**	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Residents Per Inpatient Bed	0.01	0.03	0.10	0.07	0.50	0.22	0.09	0.18
Inpatient Hospital Beds	152	134	373	208	510	284	195	184
Inpatient Admissions/Year	5,727	6,165	15,523	8,815	21,964	10,277	7,654	8,136
Patient Age	48.0	9.8	44.7	8.1	39.5	8.0	46.0	9.7
Medicaid Admissions/Total Admissions	16%	13%	18%	15%	27%	16%	18%	14%
Medicare Admissions/Total Admissions	38%	14%	32%	11%	24%	10%	35%	14%
Average Length of Stay								
Total	5.1	2.5	5.6	1.5	6.2	1.5	5.4	2.2
Age<65	3.9	2.1	4.6	1.4	5.6	1.5	4.4	2.0
Age 65+	7.0	3.4	7.6	2.1	8.1	2.5	7.2	3.1
Mortality								
Total	2.7%	1.0%	2.4%	0.7%	2.4%	0.7%	2.6%	0.9%
Age<65	0.9%	0.5%	1.0%	0.4%	1.5%	0.5%	1.0%	0.5%
Age 65+	5.4%	1.4%	5.4%	1.3%	5.6%	1.6%	5.4%	1.4%
Observations	3,099		426		183		3,708	

Note: Observations are at the hospital-year level and cover the five-year period from 1993 to 1997.

Source: NIS, 1993-97.

Table III: LOS, Mortality, and Admission Regressions Using Discrete Hospital Categories (All Diagnoses)

	Change in Dependent Variable Relative to Non-Teaching Baseline (Reference Period=April-May)		
	Risk-Adjusted LOS	Risk-Adjusted Mortality	Monthly Admissions
Minor Teaching			
Jan-Mar	0.017 (0.018)	0.023 (0.025)	-4.3 (5.1)
Apr-May			
June	-0.014 (0.020)	0.052 (0.032)	-9.3 (4.7) **
Jul-Aug	0.022 (0.019)	0.051 (0.028) *	-4.9 (6.1)
Sep-Oct	0.044 (0.020) **	0.027 (0.027)	-14.1 (6.3) **
Nov-Dec	0.050 (0.021) **	0.023 (0.028)	-36.0 (7.2) ***
Major Teaching			
Jan-Mar	0.077 (0.032) **	0.003 (0.035)	-9.5 (10.6)
Apr-May			
June	0.081 (0.045) *	0.126 (0.047) ***	-17.9 (12.2)
Jul-Aug	0.129 (0.042) ***	0.185 (0.075) ***	32.5 (12.4) ***
Sep-Oct	0.127 (0.037) ***	0.147 (0.083) *	7.4 (13.0)
Nov-Dec	0.142 (0.030) ***	0.175 (0.078) **	-37.1 (11.5) ***
Mean of Dependent Variable			
Minor Teaching Hospitals	5.51	2.61	1,294
Major Teaching Hospitals	6.22	3.09	1,830
All Hospitals (Includes Non-Teaching)	5.37	2.63	638
Observations	44,342	44,342	44,342
Adjusted R^2	0.762	0.477	0.986

,, and *** denote statistical signficance at the 10%, 5%, and 1% levels, respectively.*

Note: All regressions include fixed effects for hospital, year, and month. Standard errors (in parentheses) are heteroskedasticity robust and clustered by hospital.

Table IV: LOS, Mortality, and Admission Regressions Using Discrete Hospital Categories (High-Mortality Diagnoses)

	Change in Dependent Variable Relative to Non-Teaching Baseline (Reference Period=April-May)		
	Risk-Adjusted LOS	Risk-Adjusted Mortality	Monthly Admissions
Minor Teaching			
Jan-Mar	0.015 (0.054)	0.136 (0.109)	0.2 (1.5)
Apr-May			
June	0.002 (0.065)	0.234 (0.146)	-4.9 (1.1) ***
Jul-Aug	0.130 (0.064) **	0.270 (0.125) **	-7.0 (1.3) ***
Sep-Oct	0.232 (0.053) ***	0.151 (0.126)	-6.1 (1.1) ***
Nov-Dec	0.092 (0.054) *	0.213 (0.120) *	-0.8 (1.4)
Major Teaching			
Jan-Mar	0.210 (0.098) **	-0.112 (0.131)	-2.6 (2.5)
Apr-May			
June	0.116 (0.107)	0.322 (0.169) *	-5.1 (2.6) **
Jul-Aug	0.261 (0.076) ***	0.485 (0.274) *	0.1 (2.3)
Sep-Oct	0.375 (0.069) ***	0.492 (0.296) *	-2.6 (1.7)
Nov-Dec	0.210 (0.079) ***	0.497 (0.262) *	-3.8 (2.7)
Mean of Dependent Variable			
Minor Teaching Hospitals	9.32	10.71	208
Major Teaching Hospitals	10.26	11.10	312
All Hospitals (Includes Non-Teaching)	9.06	10.43	113
Observations	44,005	44,005	44,005
Adjusted R^2	0.567	0.319	0.971

,, and *** denote statistical signficance at the 10%, 5%, and 1% levels, respectively.*

Note: All regressions include fixed effects for hospital, year, and month. Standard errors (in parentheses) are heteroskedasticity robust and clustered by hospital.

Table V: LOS and Mortality Regressions Using Continuous Teaching Intensity (All Diagnoses)

		Change in Dependent Variable Relative to Non-Teaching Baseline (Reference Period=April-May)			
		All Admissions		High-Mortality Admissions	
		Risk-Adjusted LOS	Risk-Adjusted Mortality	Risk-Adjusted LOS	Risk-Adjusted Mortality
Linear Interactions: (Residents/Bed) x					
	Jan-Mar	0.230 (0.111) **	0.041 (0.136)	0.569 (0.342) *	-0.012 (0.467)
	Apr-May				
	June	0.232 (0.155)	0.475 (0.166) ***	0.165 (0.410)	1.010 (0.624)
	Jul-Aug	0.378 (0.141) ***	0.733 (0.256) ***	0.960 (0.326) ***	2.654 (0.945) ***
	Sep-Oct	0.324 (0.130) ***	0.470 (0.278) *	1.234 (0.344) ***	1.846 (0.996) *
	Nov-Dec	0.360 (0.114) ***	0.667 (0.261) ***	0.481 (0.357)	2.048 (0.891) **
Quadratic Interactions: $(\text{Residents/Bed})^2$ x					
	Jan-Mar	-0.162 (0.109)	-0.113 (0.165)	-0.348 (0.404)	-0.708 (0.534)
	Apr-May				
	June	-0.130 (0.165)	-0.447 (0.178) ***	0.200 (0.502)	-1.128 (0.649) *
	Jul-Aug	-0.204 (0.149)	-0.734 (0.260) ***	-0.732 (0.415) *	-3.357 (1.058) ***
	Sep-Oct	-0.121 (0.135)	-0.438 (0.294)	-0.832 (0.453) *	-2.244 (1.079) **
	Nov-Dec	-0.186 (0.122)	-0.612 (0.269) **	-0.126 (0.485)	-2.260 (1.027) **
Mean of Dependent Variable		5.37	2.63	9.06	10.43
Observations		44,342	44,342	44,005	44,005
Adjusted R^2		0.762	0.477	0.567	0.319

,, and *** denote statistical signficance at the 10%, 5%, and 1% levels, respectively.*

Note: All regressions include fixed effects for hospital, year, and month. Standard errors (in parentheses) are heteroskedasticity robust and clustered by hospital.

Figure I: Risk-Adjusted Mortality and LOS by Month (All Discharges)

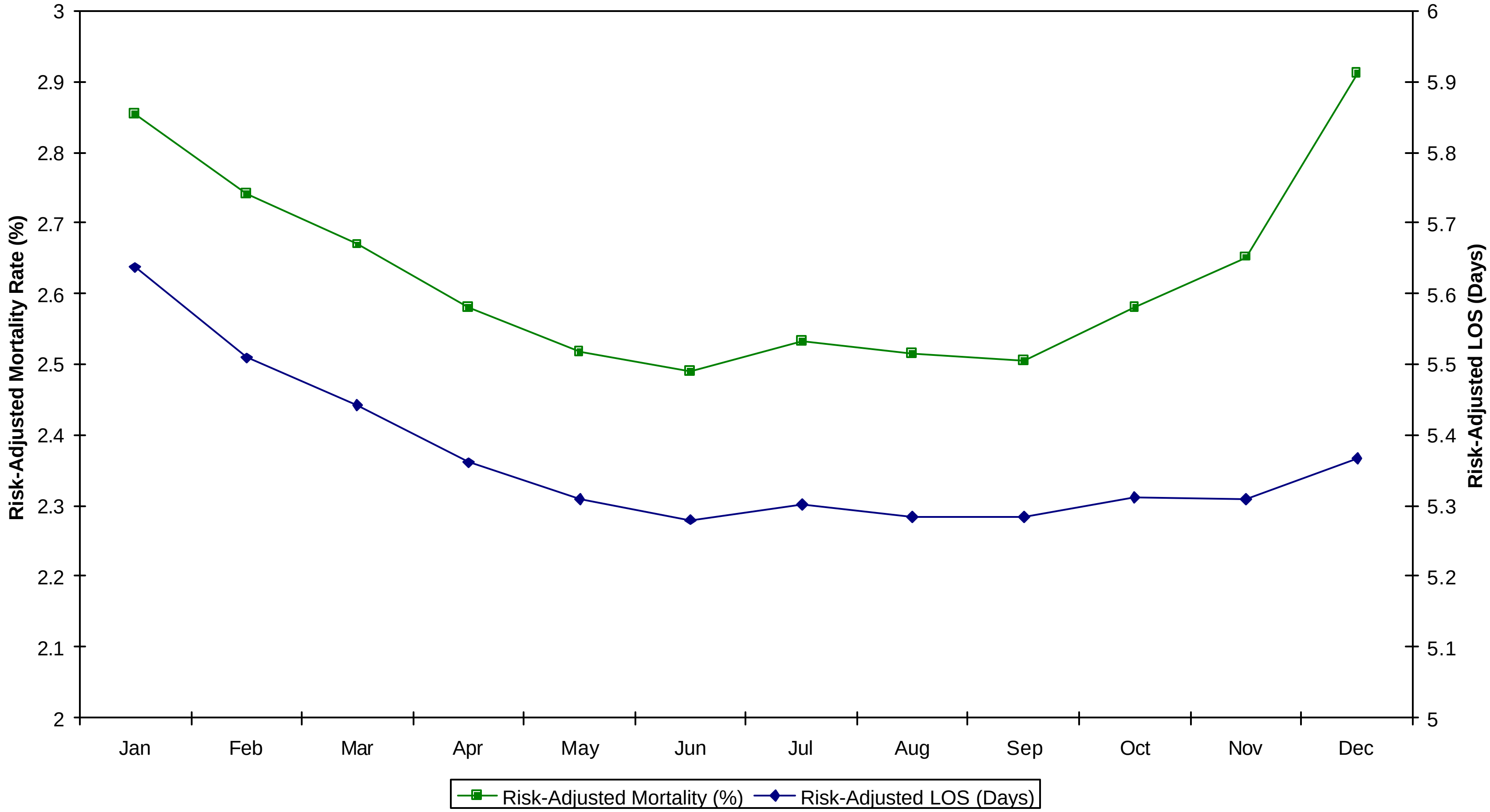
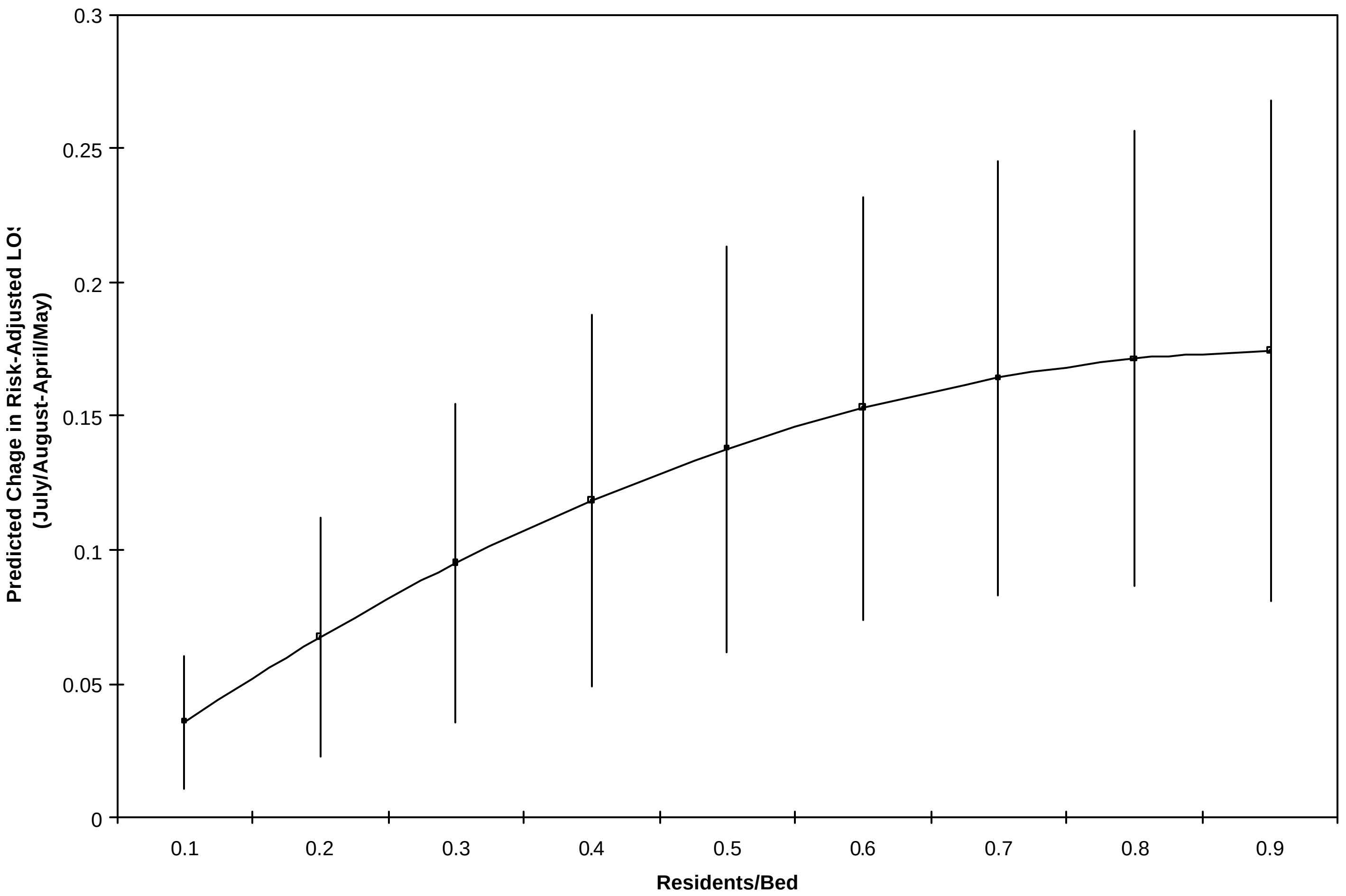
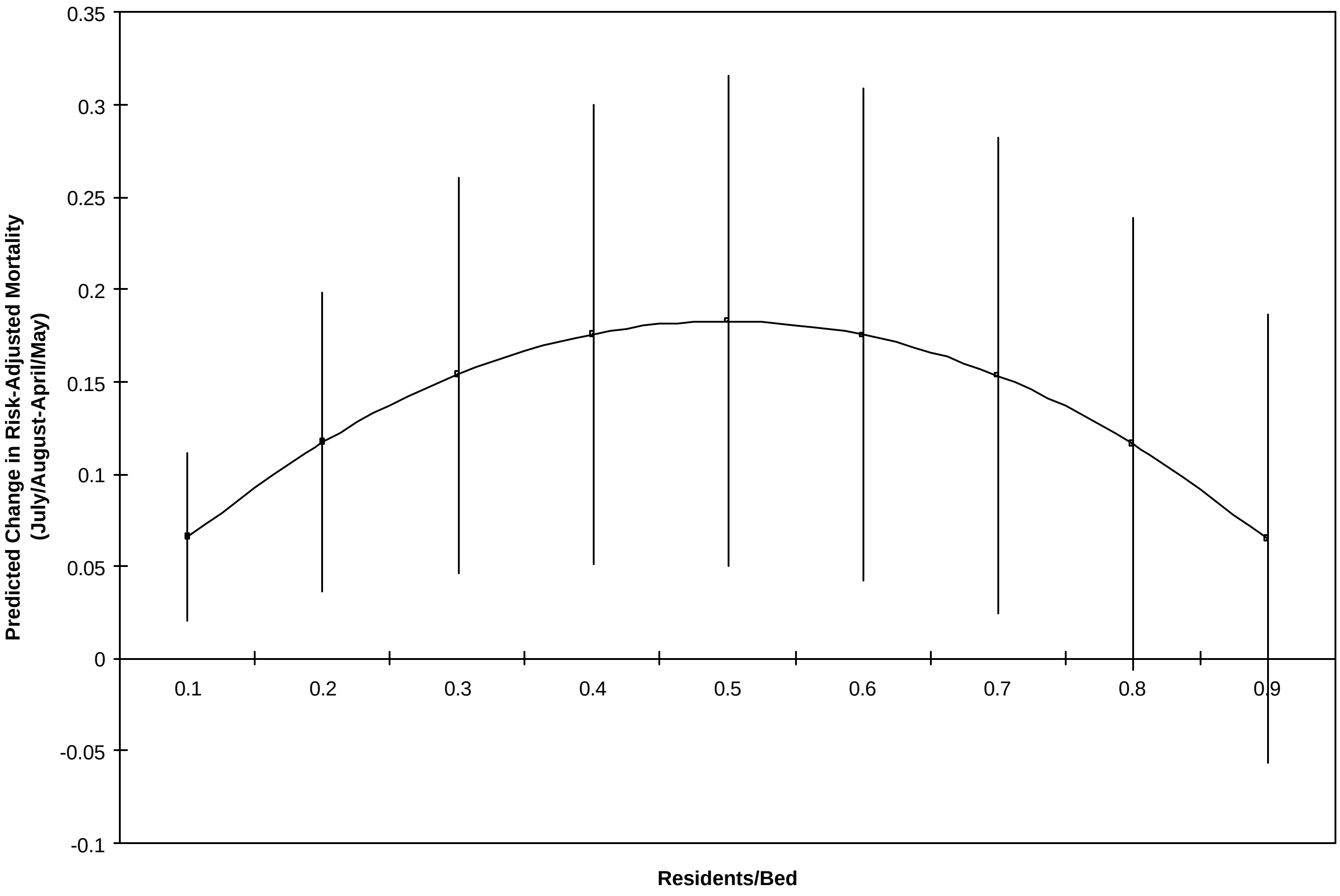


Figure II: Magnitude of LOS July Effect Using Continuous Teaching Intensity (All Admissions)



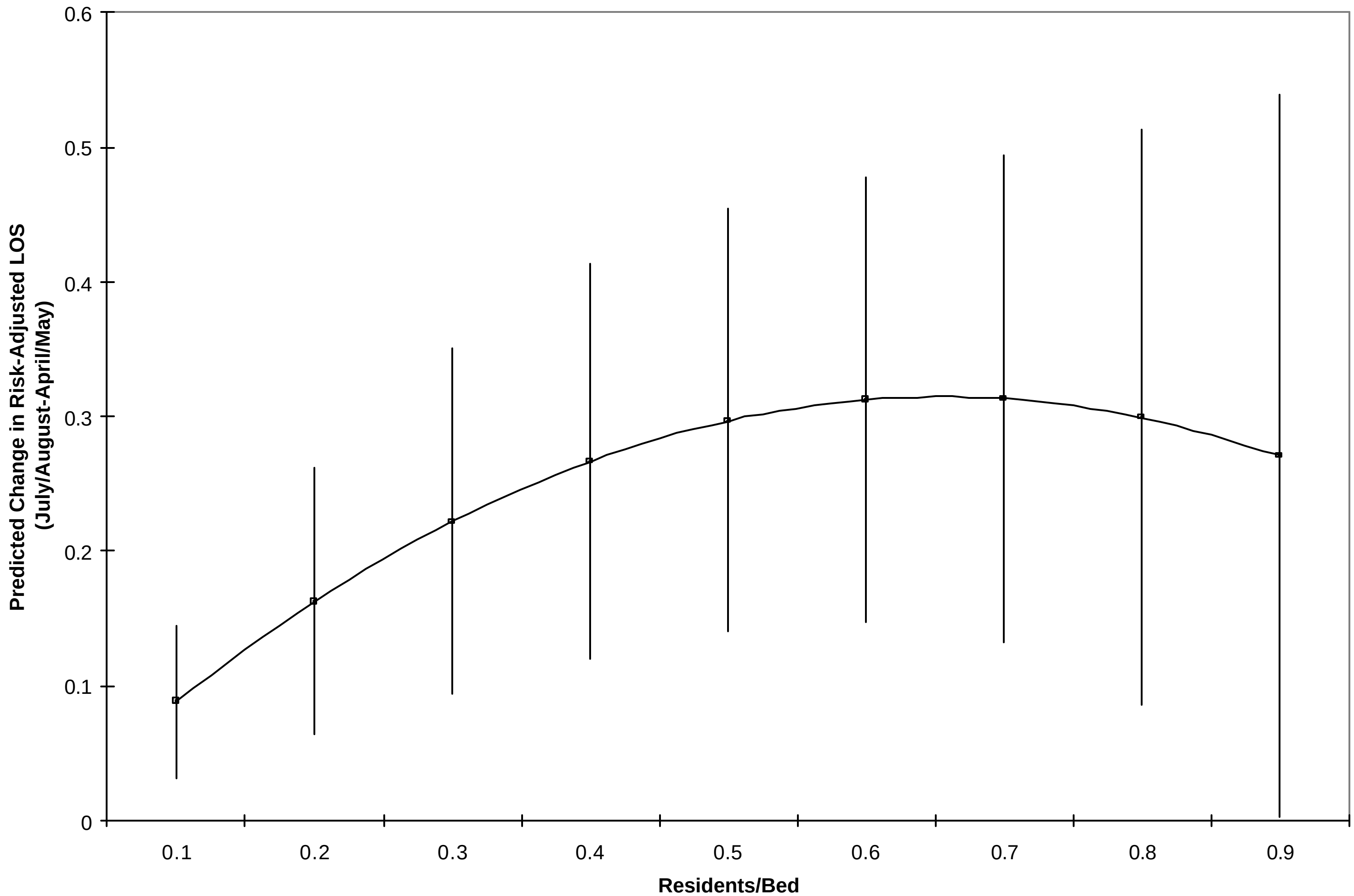
Note: Vertical bars represent 95% confidence intervals around estimates.

Figure III: Magnitude of Mortality July Effect Using Continuous Teaching Intensity (All Admissions)



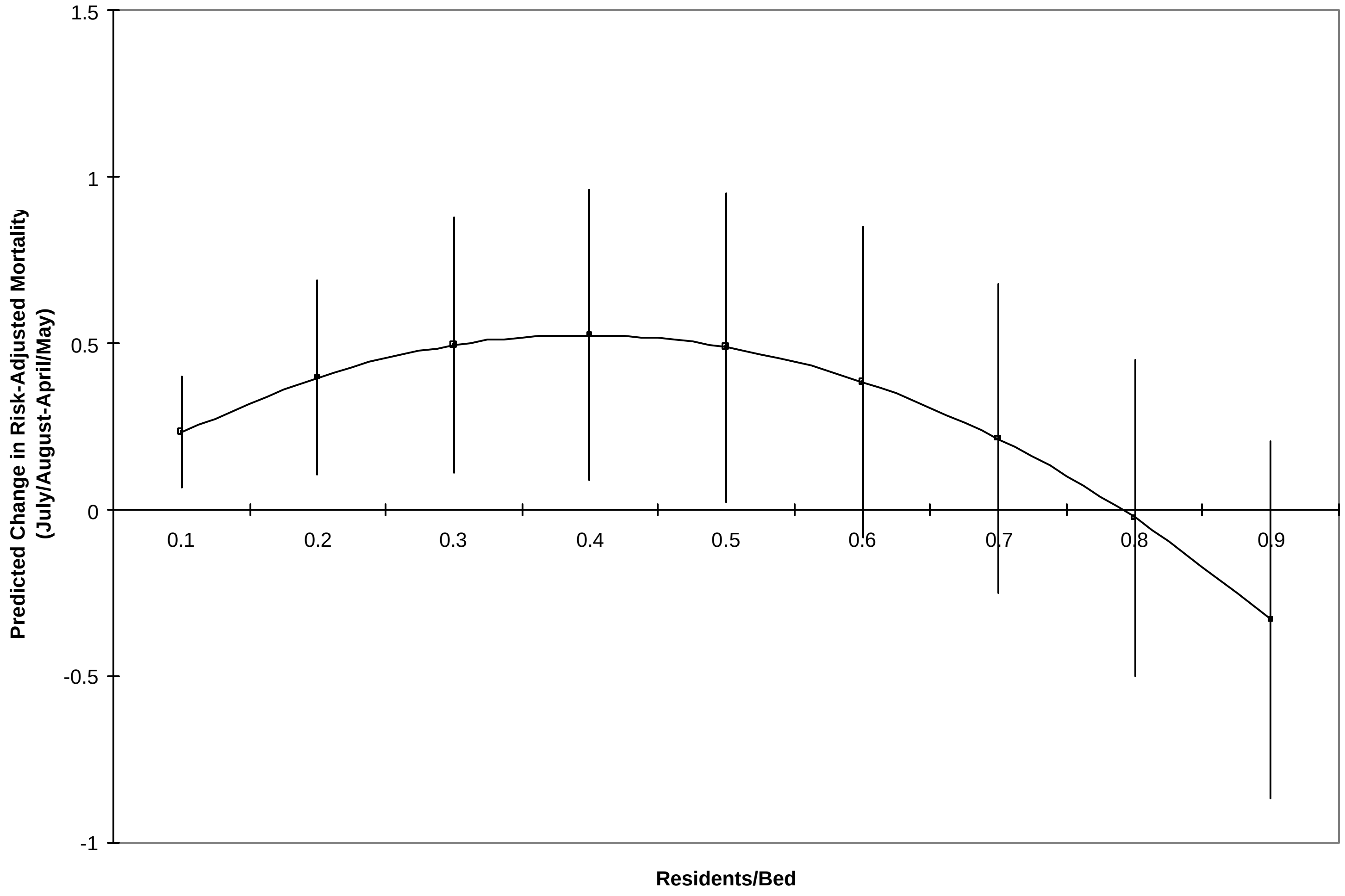
Note: Vertical bars represent 95% confidence intervals around estimates.

Figure IV: Magnitude of LOS July Effect Using Continuous Teaching Intensity (High-Mortality Admissions)



Note: Vertical bars represent 95% confidence intervals around estimates.

Figure V: Magnitude of Mortality July Effect Using Continuous Teaching Intensity (High-Mortality Admissions)



Note: Vertical bars represent 95% confidence intervals around estimates.